the usual engineering practice to so distribute the stresses that no joint tends to open under the most unfavourable conditions, though this condition is doubtless frequently neglected in flimsy structures. In order that this condition should be fulfilled, the resultant of the pressure on the base must not deviate from the centre of gravity of the base by a quantity greater than x' given in the equation $x' = \frac{I}{XS}$, where I is the moment of inertia of the base about the neutral axis or line through its centre of gravity perpendicular to the direction of the deviation of the resultant, S = the area of the base, and X = the greatest distance of a point in the base from the neutral axis on the side of the greatest pressure. In the case of a circular base $x' = \frac{\text{diameter of base}}{8} = {\cdot}625$ feet in the present instance. The wind pressure corresponding to this deviation $= \frac{196 \times {\cdot}625 \times 2240}{12{,}931} = 21{\cdot}22$ lbs. per square foot. When the wind-pressure exceeds this amount there is still the tensile strength of the cement with which the stone is bedded to resist the tendency of the joint to open on the windward side. While the introduction of a layer of cement under the stone doubtless adds to its steadiness under a wind-pressure of 30 or 50 lbs. to the square foot, it would add a very serious element of danger should the pressure ever approach that recorded at the Bidston Observatory, as the cement on the lee side would probably then be subjected to a crushing stress in excess of its strength, and by giving way would cause the column to heel over to some extent; in fact, if there was any probability of that wind-pressure being reached, it would have been safer to have omitted the cement and trusted for the ultimate stability to the far greater resistance to crushing of the granite. It would be impossible, without making assumptions unfounded on experiment, to estimate with any accuracy the value of the additional stability given by the cement in the case of moderate wind-pressures. We have, however, calculated the conditions of equilibrium, neglecting the tensile strength of the cement, as well as the bending of the stone.

On this assumption, we find that a wind pressure of 50 lbs, per square foot would cause the joint to open on the windward side as far into the base as the centre; the column would thus be standing only on the leeward half of its base, but the stability would not be endangered by this as the maximum pressure on the base at its outer edge would only amount to 40 tons per square foot, which is less than the crushing strength even of the cement. The line of the resultant pressure on the base would be at a distance of 1·472 feet from the centre, if the bending of the column is disregarded. To take into consideration the flexure of the column would involve too long calculations for our present purpose, even if the modulus of elasticity of granite had been determined with sufficient accuracy to make the results of any value, but this we believe has not yet been done. The conclusions we arrive at are as follows:—As long as the foundations remain secure, the obelisk may be frequently subjected to a wind pressure of 21 lbs. per square foot without the slightest tendency to accident; if subjected at long intervals to a pressure of 40 or 50 lbs. to the square foot, it would probably stand for an indefinitely long period until the fatigue of the cement under variations of stress or its natural decay, if that ever takes place, causes its rupture, but under a pressure of this intensity it must be borne in mind that considerable oscillation would take place, and that if the period of the gusts nearly agreed with the time of vibration of the stone it might be overturned; while if a pressure of 80 lbs. per square foot is reached it is very questionable if the survivors among the inhabitants of the neighbourhood will find it *in situ* when they have time to go and look for it.

DRAPER'S SCIENTIFIC MEMOIRS

Scientific Memoirs: being Experimental Contributions to a Knowledge of Radiant Energy. By John William Draper, M.D., LL.D. (London: Sampson, Low, and Co. New York: Harper Brothers, 1878.)

THE scientific world is to be congratulated on the accession to its literature of these memoirs constituting as they do a distinct historical sketch of the works of a physicist who is at once an ardent experimentalist and a careful theorist. As he remarks in his preface, many of his results of experimental investigation on scientific topics have been largely disseminated in European languages, and many of the conclusions they have presented have been admitted into the accepted body of scientific knowledge. The papers in which these results were published have, however, appeared from time to time in various American and English periodicals, but we now have them collected in a form in which they are accessible and convenient for reference.

The four opening memoirs seemingly occupy their position in the volume for the purpose of calling the attention of the reader to the fact that a large portion of the subject that Kirchhoff treated mathematically in a paper which appeared in *Poggendorff's Annalen* in 1860, and which at the time was considered the foundation of spectrum analysis, had already been experimentally proved and published by our author some thirteen years before. The theorist apparently ignored the work of the experimentalist, and the claim of the one to priority in regard to the enunciation of certain fundamental principles of spectrum analysis is now on the best of evidence disputed by the other. The titles of these first four memoirs and their dates of original publication will give an idea of the indictment framed against Kirchhoff which appears in a note appended to the last of them. They are—

I. Examination of the radiations of red-hot bodies. The production of light by heat, published in 1847.

II. Spectrum analysis of flames. Production of light by chemical action, published in 1848.

III. On invisible fixed lines in the sun's spectrum detected by photography, published in 1843.

IV. On the nature of flame, and on the condition of the sun's surface, published in 1858.

Controversy regarding priority of discovery is always distasteful, and the indictment against Kirchhoff is a heavy one, but the offence might have been charged also against those scientific writers who, careless of history, have been accomplices in doing Draper an injustice. But turning to the more agreeable side of the subject of these memoirs we find that Draper fixed the temperature at which solid

bodies emit light with heat to be 977°, and shows experimentally that as the temperature of an incandescent body rises it emits rays of an increasing refrangibility; also that the amplitude of any particular vibration increases with the temperature, and that to every particular wave-length there belongs a particular colour. But even more remarkable are the deductions he makes regarding light and heat, deductions which, though evident *now*, perhaps, in the present state of knowledge, had by no means *then* the appearance of undoubted truths. He boldly asserted that light and heat were effects of radiation and not forces existing in the radiations themselves.

It is, however, with photographic research that the name of Draper is most generally linked; and as his researches in this line commenced in 1837, two years before the announcement of Daguerre's and Fox Talbot's discoveries, his claim to be considered one of the pioneers in photography admits of no contravention. In his memoir on "Studies in the Diffraction Spectrum" we read: "Several years before the commencement of the discovery of photography by Daguerre and Talbot (1839), I had made use of the process for the purpose of ascertaining whether the so-called chemical rays exhibited interference, and in 1837 published the results in the *Journal* of the Franklin Institute, Philadelphia (July, 1837, p. 45). In this, as will be seen by consulting that publication, I was successful." In his memoir of 1843, he describes the mode in which he photographed the spectrum, from the blue to the ultra-violet, and from near C in the red region to a point some distance below the limit of visibility. The apparatus he employed would at the present time be considered, perhaps, somewhat rude, but, as is well known, the roughest appliances in the hands of a true philosopher are sufficient even for delicate experiment. Thus, in photographing his spectrum we find that he worked before the days of collimating lenses, and with a consequent feebleness of light which was a serious matter when the slow (as compared with that now extant) process of Daguerre was employed for registering the impressions of the radiations. Half an hour's exposure was not too long to give to obtain a developable image, whereas now as many seconds as he gave minutes, with the same size of spectrum and width of slit, would be more than ample. The method by which Draper registered the lines in the red and ultra-red regions is fully treated of in his fifth memoir. The plate received a preliminary exposure to white light, and was then exposed to spectrum; or feeble daylight was allowed access to the plate whilst being similarly exposed; the result, on development by mercury, being that the dark lines in these regions were registered as light lines on a dark background, instead of as dark lines on a white background. This action Draper, Claudet, and others ascribed to the antagonism of the blue and red rays which are found in white light, and heads his memoir "Interference of radiations" in consequence. Till last year this view of the antagonism of rays was accepted as existent, when it received a blow, and probably a final one, from the announcement of the experimental proof that this action was produced through the spectrum possessing the power of accelerating the oxidation of the compound of silver which had been altered by light, and which, when so changed became undevelopable. Whatever may be the explanation of this phenomenon, we have in Draper's photographs of the least refrangible region a gigantic feat, considering the date at which it was performed. Though recent methods may outstrip the more antiquated one as regards rapidity of execution, yet it is due to him to acknowledge that he has long priority in showing that chemical action was not confined to the least refrangible end of the spectrum. As regards the application of photography to portraiture, to our author seems to belong the honour of having taken the first portrait by the Daguerreotype process, and the arrangements adopted for the purpose read rather comically in these days of quasi-instantaneous pictures. In his memoir, "On Taking of Portraits by Photography," he says:—"On a bright day, and with a sensitive plate, portraits can be obtained in the course of five or seven minutes in the diffused daylight. . . . Difficult parts of the dress . . . require intervals (exposure) differing considerably, to be fairly copied, the white parts of a costume passing on to solarisation before the yellow or black parts have made any decisive representation. We have therefore to make use of temporary expedients. A person dressed in a black coat and open waistcoat of the same colour must put on a temporary front of a drab or flesh colour, or, by the time that his face and the dark shadows of woollen clothing are evolved, his shirt will be blue, or even black, with a white halo around it." We are sure that the author cannot have regretted the supercession of a process which entailed such "dodging" to render a portrait practicable, more particularly at the time when he sat for the photograph from which the admirable portrait forming the frontispiece was engraved.

To this same memoir we have also an append in which it is shown that Dr. Draper had priority in taking a photograph of the moon; and when it is considered that the exposure was twenty minutes, and the diameter of the image about one inch, it would not be surprising had it lacked in detail. By an extract from the minutes of the New York Lyceum of Natural History we learn that in this photograph we have "a distinct representation of the moon's surface."

To yet another discovery of Draper's we must refer, since, like some others of his, it has been re-discovered quite recently. He says, in his description of the Daguerreotype process, "On these principles" (he alludes to the different photographic effects produced by different rays of light) "it is plain that an achromatic object-glass is by no means essential for the production of fine photographs; for if the plate be withdrawn at a certain period when the rays that have a maximum energy have just completed their action, those that are more dispersed but of slower effect will not have had time to leave any stain. We work, in fact, with a temporary monochromatic light." With a cigar-box as camera and a spectacle-lens as an objective he tested his theory, and found that on this principle he could photograph an engraving, with all its finest details present. The similarity between Janssen's use of an uncorrected lens for solar work and this is apparent.

Mixed up with photography is actinometry, and here we find that Draper not only invented the chlor-hydrogen photometer, which depends on the combination of chlo-

rine and hydrogen when acted upon by radiations, but that he also used it practically, though not with such nicety of method as subsequently employed by Bunsen and Roscoe. He also invented the ferric oxalate photometer, dependent on the reduction of this ferric compound to the ferrous state and the liberation of carbonic acid. In both of the foregoing we have a measure of the *quantity* of the radiations which these mixed gases, or solution, select. On this particular subject of selective absorption, when chemical action takes place, Draper experimented most fully. He showed, for instance, that the sensitiveness of the surface of a Daguerreotype plate is at its maximum when of a yellow tint, owing to the absorption of the blue rays, and conclusively shows that when it is of a blue tint that these same rays are largely reflected. In fact, he announced, with all the authority of a successful experimentalist, that for the production of chemical action in a compound by any particular ray, the absorption of that ray by the compound was an absolute necessity. In late years we have had several rediscoveries of this important truth, and probably it will be rediscovered again and again, notwithstanding the publication of these memoirs.

We have not space to do more than to mention the memoirs on the "Distribution of Heat in the Spectrum," on "The Chemical Force in the Spectrum" (both titles of which, by the by, are inexact, as Draper himself was the first to prove), and on "The Supposed Magnetic Effects Produced by the Violet Rays," all of which are important contributions to science, as are also those on "The Cause of the Flow of Sap in Plants, and the Circulation of the Blood in Animals," and "On the Decomposition of Carbonic Acid Gas by Plants in the Prismatic Spectrum." All these have been treated in a masterly manner, and the results lucidly and tritely recorded. Reading these memoirs leads us to the conclusion that we have in Draper a successful experimenter, who has been perhaps too little appreciated in the world owing to his too great modesty in neglecting to call attention to the facts he has observed, and to claim for himself honour where the honour was due. Like other men of mark in science, the ardent pursuit of it was undertaken through what might be termed an accident. He tells us in his preface that happening to see a glass containing some camphor, portions of which had been caused to condense in very beautiful crystals, he was induced to read everything he could obtain respecting the chemical and mechanical influence of light, adhesion, and capillary attraction; the experiments he made in connection with these subjects being contained in the volume before us. His thoughts being thus directed to physiological studies, he published papers on these topics in the *American Journal of Medical Sciences*, which created such a favourable impression that he was appointed, in 1836, Professor of Chemistry and Physiology in Hampden Sidney College, Virginia. He afterwards was appointed to a similar chair at New York University, which, we believe, he at present holds.

It would be travelling out of our province to do more than call attention to Dr. Draper as the author of "A Treatise on Human Physiology," "The History of the Intellectual Development of Europe," "The History of the American Civil War," and of "The History of the Conflict of Religion and Science," works which have met with well-merited success, and which show the varied bent of his mind.

The history of the Rumford medal fund held in trust by the Royal Society, and the awards made by this body are too well known to need repetition; but it is not equally well known that a similar medal fund was founded in the United States by Rumford, and is held in trust by the American Academy of Arts and Sciences. The medals were to be awarded for "the most important discovery or improvement relating to light and heat that had been made during the preceding two years in any part of America." The awards of the American Rumford medals have been made few and far between, and till 1876 may be said to have been given for inventions rather than discoveries. At this date the medal was awarded to Dr. Draper (as the medal itself records) "for researches on radiant energy." Had he been an European there can be little doubt but that he would have received one of our English medals years ago, and that his name would have been in the same list with those of Leslie, Fox Talbot, Fresnel, and Faraday. As it is he has the honour of being the first recipient of the American Rumford medal which has ever been awarded for pure scientific research.

A CATECHISM OF BOTANY

A First Catechism of Botany. By John Gibbs, of the Essex and Chelmsford Museum. (Chelmsford: Edmund Durrant and Co. London: Simpkin, Marshall, and Co.)

THIS little book is in its way quite a curiosity. It is a survival of a method of instruction which was very popular in its day, but which it is to be hoped—notwithstanding that Magnall's "Questions" is still said to be a good property—even in country towns like Chelmsford, is on the road to extinction. Catechisms originated in the necessity of giving some uniformity and precision to oral religious instruction. Their great merit is of course that they remove all responsibility from the teacher, and merely require that their formulæ should be taught with patience and perseverance. They render unnecessary, indeed even undesirable, any knowledge of the subject on the part of the teacher, and hence it is easy to see the reason of their popularity amongst persons engaged in education, and who, possessed of no scientific training, are yet anxious to get credit for teaching scientific subjects. Mr. Gibbs has evidently felt some uneasiness on this head, and points out accordingly in his preface that:—

> "The answer to every question may be verified by examination of the plant itself in all its parts to which reference is made. Only in such a way can this catechism be made useful, and by such criticism its value will be ascertained."

But the insidious influence of the purely dogmatic method makes itself but too evident in the next sentence, which is surely the strangest ground of recommendation ever urged for a scientific book:—

> "In its original form it was admitted to the International Exhibition of 1871, which contained nothing but what the Committee of Selection approved as excellent."